observations which appear to him to confirm what he had advanced on a former occasion touching the dispersive powers of the eye.

On the Composition of Emery. By Smithson Tennant, *Esq. F.R.S.* Read July 1, 1802. [*Phil. Trans.* 1802, *p.* 398.]

The ultimate results of the experiments made on this substance, which it seems had never before been properly analysed, are—that 25 grains contain 12½ grains of argillaceous earth, 2 of silex, and 8 of iron; that 1 grain was not dissolved, and that the remainder, being 1½ grain, was lost in the process. Another process gave the same components, but in somewhat different proportions. These ingredients being very similar to those found by Mr. Klaproth in Diamond spar, it is thought that emery is essentially a substance of the same nature, with perhaps a somewhat greater proportion of iron.

Quelques Remarques sur la Chaleur, et sur l'Action des Corps qui l'interceptent. Par P. Prevost, *Professeur de Philosophie à Genève, &c. Communicated by* Thomas Young, *M.D. F.R.S.* Read July 1, 1802. [*Phil. Trans.* 1802, *p.* 403.]

The remarks here brought forward relate chiefly to Dr. Herschel's experiments on the solar and terrestrial rays that occasion heat, published in the Philosophical Transactions for the year 1800, and are meant to rectify some anomalies which appear in their results. The paper consists of two parts: the first being the observations on Dr. Herschel's experiments, and some new ones, with inferences deduced from them; and the second the exposition of a theory, which the author thinks may reconcile all contradictions.

In the first part he sets out with briefly stating the manner in which Dr. Herschel conducted the experiments he made, in order to estimate, by the indications of different thermometers, the quantity of heat transmitted through various substances, compared with the heat afforded by direct rays from different luminous bodies, or more properly sources of heat. Here the author soon starts a difficulty concerning the mode of estimating the intercepting power of the substances used in the experiments. As these experiments consist of a series of observations made progressively at intervals of one minute between each other, it follows that the ratio Dr. Herschel adopted between the heat produced by direct rays, and those transmitted through coloured media, is not, as he imagined, a constant proportion, he having uniformly deduced his inferences from the differences between the initial and the final degrees of heat; whereas, had he attended to the intermediate observations, he would have found that each of them would have afforded a different ratio.

Having maturely considered this subject, the author, adverting to this circumstance of the various proportions of heat progressively yielded in these experiments, observes, that it can hardly be conceived why the faculty of transmitting and intercepting heat should

thus vary in any substance, merely because it has transmitted or intercepted it for a greater or less length of time. Hence he thinks it essential to have recourse to some permanent rule from which the results may in all cases be accurately derived, and which, when the phænomena do not correspond, may lead us to the investigation of some other cause. Such a law has been deduced from direct experiments, and implies that a body placed in a medium of a constant temperature, becomes heated or cooled in such a manner, that the differences of its heat from that of the medium are in a geometrical progression, while the times of heating or of cooling are arithmetically proportionate. It will readily be perceived in what manner it is practicable to deduce from the two progressions mentioned in this law, a third progression, which will apply to the intermediate steps of any series of observations.

This law, when adapted both to Dr. Herschel's experiments and to some new ones here described, is found to apply with singular accuracy through the three or four first minutes of increasing heat; but after this period the series manifestly varies, the increase of heat by computation according to the law falling progressively short of that indicated by the thermometers. The author is at considerable pains to explain this anomaly, and at length ascribes it to the heat accumulated in the intercepting body, which renders it in a manner a new source of heat, the emanation from which, it must be admitted, cannot but cooperate with the transmitted rays, to raise the thermometers near it.

If the progress of this accumulation of heat be perfectly regular, its effect will be confounded with that of the transmitted rays, as was actually found to be the case when a thin plate of talc was used as an intercepting medium. The cause of this difference is ascribed chiefly to the thickness of that medium, and in some measure also to the weakness of the source of heat. It will scarcely be necessary to explain the operation of these concurrent causes, it being obvious that the greater the bulk of a body, the greater will be the accumulation it admits of, and the greater the source of heat, the more rapid will be this accumulation.

The next object of inquiry is how long an experiment should last for the thermometer to acquire the maximum of heating, that is, the temperature of the source of heat, or medium in which it is immersed. Here the experiments can be made only on direct heat, since the intermediate body containing accumulated heat, might, and probably does in most cases, continue to emit this heat after the thermometer has arrived at the maximum, that is, the temperature of the source of heat. In the direct heat of the sun this maximum was obtained in little more than 12′.

The author hereupon examines a number of Dr. Herschel's experiments, in which he mentions only the initial and final degrees of the thermometer. After showing what the mean ratio is between the degrees computed for the progression of the differences, and those determined by observation, which he finds is as 13 to 10, he deter-

mines the constant heat of a medium by the following proportion. The difference between this heat, and each of the numbers given by observation (that is, the initial and final observation), are to each other as the first term of the progression is to the sixth; that is to say, as the numbers 13 and 10 raised to the fifth power.

These comparisons between his results and those Dr. Herschel had derived from the same experiments, have led our author to several remarks, in which the above-mentioned law, and the circumstance of the accumulation of heat in the intercepting media, are applied to various phænomena and computations, and likewise to some experiments of the same nature described by Prof. Pictet in his Essay on Fire. The deviations here observed are in most cases ascribed to the thickness of the intercepting substances, and to the distances between them and the thermometers.

The second part, which relates to the theory from which depends the law of the increments of heat, as deduced from direct observations, is introduced by a brief statement of the historical facts that have led to the contemplation of this subject. Bacon first proposed the question, whether heated bodies, which are obscure and opake, are similar in their effects to the radiant bodies? Several philosophers, such as Lambert, Saussure, and Pictet, have by various experiments determined in favour of the affirmative; and it has even been proved that the velocity of heat, independent of light, is no less than 69 feet in an instant of time not apparently divisible.

Bacon likewise asked whether cold might not, as well as heat, acquire intensity by means of mirrors or refracting glasses? Our author, without mentioning the well-known experiments of the Academy del Cimento on this subject, proceeds at once to those of Prof. Pictet, who proved the affirmative as to the fact, but yet thought that the cause ought to be ascribed not to the reflected cold, but to the reflection of heat in opposite circumstances; by which he seems to understand that heat in this instance escapes reciprocally from the thermometer towards the cooler substance. He here substitutes a moveable equilibrium, to the immoveable one usually admitted by philosophers; and this he thinks fully explains the identity of the phænomena according to his theory, which implies an equal apparent dispersion of heat and cold.

This theory is as follows:—Fire is a discrete and agitated fluid; every molecule of free fire is moved with great velocity: some molecules move one way, some another, so that a hot body throws out calorific rays in every direction. And these molecules have sufficient distance between them to admit two or more currents to cross each other without being impeded in their course. This character of fire being clearly understood, it must be evident (says our author) that if we suppose two neighbouring spaces to contain a certain quantity of it, there must be continual changes between them. If the fire is equally abundant in each, the changes will be equal, and an equilibrium will be produced: if one of the spaces contain more fire than the other, the changes will be unequal; but after a sufficient time

the continual repetition of these changes will likewise produce an equilibrium. According to these principles he undertakes to explain all the laws of increasing and decreasing heat; he supplies us with various examples as to the application of them, and shows how this theory coincides with the general law laid down in the first part of the paper.

The drift of this treatise will be still further illustrated, if we attend to the following recapitulation given us by the author of the leading principles deduced from the various parts of his investigation.

1. The effect of a constant source of heat upon the thermometer is not proportional to the heat of that source.

2. We nevertheless possess a method of determining the heat of the source by its effect on the thermometer, because we know the law this effect follows in its successive increments.

3. This method is the only one that ought to be employed when it is required to compare two sources of heat, according to their effect in a limited time, less than that which is necessary to produce the maximum of the effect.

4. In the case of transmitted heat, we must distinguish that which is immediately transmitted, from that which is added by the transmitting body after it becomes heated.

5. If we neglect making the distinction, the interception of heat attributed to the intercepting body is only an inferior limit or minimum; so that it remains undetermined whether the interception has not been much greater, or even total.

6. By applying these principles to Dr. Herschel's experiments, a more exact appreciation may be obtained; it is, however, governed by some accessory circumstances, which have not yet been determined.

7. In those experiments the apparent difference between the interception of heat and of light by the same substances, does not afford any fair conclusion respecting the difference or the identity of light and heat.

8. The law mentioned in the first part of the paper is not only proved by direct experiments, but also by its agreement with the true theory of the earth.

Lastly. This theory is established upon various facts, entirely different from the above law, and it is the only one which agrees with the general phænomena of nature.

Of the Rectification of the Conic Sections. By the Rev. John Hellins, *B.D. F.R.S. and Vicar of Potter's-Pury, in Northamptonshire.* Read July 8, 1802. [*Phil. Trans.* 1802, *p.* 448.]

This, it is to be observed, is only the first part of a more extensive work, and relates merely to the rectification of the hyperbola. After a few strictures on the necessity of not relaxing in our endeavours to improve the method of fluxions, to which the author asserts few additions have been made since its first discovery by the immortal